

this he is supported by such authorities as Sachs and Strasburger; but it is impossible to say anything at present as to the form and arrangement of the micellæ of protoplasm beyond this, that they do not so act upon polarised light as to suggest that they are crystalline. Full details on this subject, as well as a vast amount of other information, is given in the treatise on the Microscope (second edition, 1877), which Nägeli wrote together with Schwendener; fortunately an English edition of this important work may soon be expected to appear.

In tracing the development of Nägeli's theory, it has been necessary to depart from the chronological order of his works. In the years between 1858 and 1868 he published his *Beiträge zur wissenschaftlichen Botanik*, which include several important works, for the most part anatomical. In the first number there is an elaborate paper on "The Arrangement of the Fibro-vascular Bundles and the Mode of Growth in the Stem and Root of Vascular Plants," which is important as containing a purely morphological classification of the different forms of tissue of which these organs consist. This is followed by a detailed account, in the fourth number, of the mode of growth in thickness and of the arrangement of the fibro-vascular bundles in the stem among the Sapindaceæ, and this number also contains Nägeli's well-known investigation into the mode of development and growth of roots, in which Leitgeb was associated with him. This publication has a further interest connected with it, in that Schwendener's first papers on what is now known as his Lichen-theory appeared in it.

During this period Nägeli frequently contributed papers (the *Botanische Mittheilungen*) on a variety of subjects of botanical interest to the *Proceedings of the Bavarian Academy*, an activity which continues up to the present time. Allusion has already been made to some of these, and it would be worth while, did space permit, to give an account of most of them. Among the more important the following may be mentioned:—"On the Sieve-Tubes of Cucurbita," "On the Proteid Crystalloids of the Brazil-nut," "On the Development of Varieties," "A Theory of Hybridisation." Of late years Nägeli has turned his attention more especially to the study of the chemical composition and vital processes of the lower Fungi, such as Yeast and Bacteria. Among the interesting results obtained is the discovery, in yeast-cells, of a ferment (invertin) which converts cane- into grape-sugar, and of peptones. But the real importance of these researches only became apparent on the publication of two larger works, viz.: "The Lower Fungi in their Relation to Infectious Disease" (1877), and "A Theory of Fermentation" (1879). It is of course impossible to give here anything like a satisfactory account of the contents of these two books. The first treats fully of the important part played by Bacteria in infection and contagion, showing, in fact, that these organisms are the causes and carriers of the various forms of disease. In the second, after an exhaustive account of the process of alcoholic fermentation has been given, a new theory of it is propounded, based, not upon chemical principles, like that of Liebig, but upon the principles of molecular physics. Fermentation is defined as being "the communication of the oscillations of the molecules, groups of atoms, and atoms of the substances composing the living protoplasm to the molecules of the fermentable substance, in consequence of which the equilibrium of the molecules of that substance is disturbed, and decomposition is the result." It is also pointed out that, in the case of yeast, the sugar is to some extent decomposed within the cells, but for the most part outside them.

Though this account of his works is but little more than an enumeration of them, yet it will suffice to show how important are Nägeli's contributions to botanical science in the departments of morphology, anatomy, and physiology, not merely as additions to the accumulated

store of facts, but as new generalisations from those facts, and as opening up fields for future research.

SYDNEY H. VINES

### PROF. TAIT ON THE FORMULA OF EVOLUTION<sup>1</sup>

ANOTHER point to which I ought thus early to direct your attention is the necessity for perfect definiteness of language in all truly scientific work. Want of definiteness may arise from habitual laziness, but it much more commonly indicates a desire to appear to know where knowledge is not. Avoid absolutely all so-called scientific writings in which (as Clerk-Maxwell said) the attempt is made to "give largeness of meaning" to a word by using it sometimes in one sense and sometimes in another. It is true that we may thus economise in our language, and avoid the necessity for introducing new and hard terms. But it would be a most expensive and pernicious economy. It is only a blockhead who could object to the use of a new term for a new idea.

Our only source of information in physical science is the evidence of our senses. To interpret truly this evidence, which is always imperfect and often wholly misleading, is one of the tasks set before Reason. It is only by the aid of reason that we can distinguish between what is physically objective, and what is merely subjective. Outside us there is no such thing as noise or brightness:—these no more exist in the aerial and ethereal motions, which are their objective cause, than does pain in the projectile which experience has taught us to avoid. You will find many prominent ideas, relics of a less enlightened age, from which Natural Philosophy has not yet wholly shaken itself free, which owed their existence solely to the confusion of the subjective with the objective.

With observation and experiment as our sole sources of information we have no right, in physical science, to introduce *à priori* reasoning. We may (unprofitably of course) speculate on what things might have been, but we must not dogmatise on what they ought to have been; we must simply try to discover what they are.

For aught that we can tell, the properties of matter, and physical laws in general, might have been other than we find them to be. How can any one of us tell whether his conscious self might not have been associated in life with the body of an Eskimo or of a New Zealander, instead of with what he (no doubt) considers its much preferable tenement? Speculations of such a kind must always be wholly unproductive and unprofitable, but for all that we cannot but allow that they are not intrinsically absurd.

Some years ago a critic of Mr. Herbert Spencer's Philosophy happened to quote from a book of mine the remark I have just made (that the properties of matter might have been other than we find them to be). Mr. Spencer's observation on this point is highly instructive. Had he not been a severely grave philosopher I should have taken it for a joke. He said, "Does this express an experimentally ascertained truth? If so, I invite Prof. Tait to describe the experiments."<sup>2</sup> Mr. Spencer has quite recently published a species of analytical inquiry<sup>3</sup> into my "mental peculiarities," "idiosyncrasies of thought," "habits of mind," "mental traits," and what not. From his illustrative quotations it appears that some or all of these are manifested wherever there are differences between myself and my critic in the points of view from which we regard the elements of science. Hence they are not properly personal questions at all, but

<sup>1</sup> Part of an Introductory Lecture delivered October 26, 1880.

<sup>2</sup> In my letter (NATURE, vol. ix. p. 402) will be found an illustrative anecdote, which Mr. Spencer declares to be "not to the point." A great scientific man, to whom I showed the correspondence, remarked that Mr. Spencer must be the only man in England who could not see the perfect appositeness of the anecdote.

<sup>3</sup> Appendix to *First Principles, dealing with Criticisms*. (Williams and Norgate, 1880.)

questions specially fitted for discussion here and now. I may, therefore, commence by inquiring what species of "mental peculiarity" my critic himself exhibited when he seriously asked me whether I had proved by *experiment* that a thing might have been what it is not!!

The title of Mr. Spencer's pamphlet informs us that it deals with *Criticisms*; and I am the first of the subjects brought up in it for vivisection, albeit I have been guilty (on Mr. Spencer's own showing) only of "*tacitly*" expressing an opinion! Surely my vivisector exhibits here also some kind of "mental peculiarity." Does a man become a critic because he quotes, with commendation if you like, a clever piece of analysis or exposition published by another?

In NATURE for July 17, 1879, I reviewed Sir E. Beckett's able little book, "Origin of the Laws of Nature," and as an illustration of that author's method I said:—

"He follows out in fact, in his own way, the hint given by a great mathematician (Kirkman) who made the following exquisite translation of a well-known definition:—

"'Evolution is a change from an indefinite, incoherent, homogeneity to a definite, coherent, heterogeneity, through continuous differentiations and integrations.'

"[Translation into plain English]—'Evolution is a change from a nohowish, untalkaboutable, all-alikeness, to a somehowish and in-general-talkaboutable not-all-alikeness, by continuous somethingelsifications and stick-togetherations.'"

Later in my article occurs the following paragraph, which also is quoted by Mr. Spencer:—

"When the purposely vague statements of the materialists and agnostics are thus stripped of the tinsel of high-flown and unintelligible language, the eyes of the thoughtless who have accepted them on authority (!) are at last opened, and they are ready to exclaim with Titania

"'Methinks 'I was enamour'd of an ass.'"

The translation is from Kirkman's remarkable work, "Philosophy without Assumptions," which at that date I had just read with pleasure and profit. Humiliating as the confession may appear, I there saw Mr. Spencer's "Formula" for the first time, and I did not notice the title given to it. Hence, in quoting it from Kirkman, I very naturally called it by its proper name, a "Definition." For this I have incurred the sore displeasure and grave censure of the inventor of the definition. It seems I should have called him the *discoverer of the formula*! Now this is no petty quibble on words. It involves, as you will see immediately, an excessively important scientific distinction, to which your attention cannot be too early directed.

Mr. Spencer complains that an American critic (whose estimate is "*tacitly*" agreed in by Mr. Matthew Arnold) says of the "Formula of Evolution":—"This may be all true, but it seems at best rather the blank form for a universe than anything corresponding to the actual world about us." On which I remark, with Mr. Kirkman, "Most just, and most merciful!" But mark what Mr. Spencer says:—

"On which the comment may be that one who had studied celestial mechanics as much as the reviewer has studied the general course of transformations, might similarly have remarked that the formula—'bodies attract one another directly as their masses and inversely as the squares of their distances,' was at best but a blank form for solar systems and sidereal clusters."

We now see why Mr. Spencer calls his form of words a *Formula*, and why he is indignant at its being called a *Definition*. He puts his Formula of Evolution along-side of the Law of Gravitation! Yet I think you will very easily see that it is a definition, and nothing more. By the help of the Law of Gravitation (not very accurately quoted by Mr. Spencer) astronomers are enabled to

predict the positions of known celestial bodies four years beforehand, in the *Nautical Almanac*, with an amount of exactness practically depending merely upon the accuracy of the observations which are constantly being made:—and, with the same limitation, the prediction could be made for 1900 A.D., or 2000 A.D., if necessary. If now Mr. Spencer's form of words be a formula, in the sense in which he uses the term as applied to the Law of Gravitation, it ought to enable us to predict, say four years before-hand, the history of Europe, with at least its main political and social changes! For Mr. Spencer says that his "formula" expresses "all orders of changes in their general course,—astronomic, geologic, biologic, psychologic, sociologic"; and therefore "could not possibly be framed in any other than words of the highest abstractness."

Added, November 11, 1880.

Mr. Kirkman has lately "discovered a formula" more general than that of Evolution, the "Formula of Universal Change." Here it is:—

"Change is a perichoretical synechy of pamparal-lagmatic and porroteroporeumatical differentiations and integrations."

Even to this all-embracing formula, with Mr. Spencer's leave, I would apply the humbler but fitter term "*definition*."

Of Mr. Spencer's farther remarks there are but three which are directed specially against myself. (Mr. Kirkman is quite able to fight his own battles.) He finds evidence of "idiosyncrasies" and what not, in the fact that, after proclaiming that nothing could be known about the physical world except by observation and experiment, I yet took part in writing the "Unseen Universe"; in which arguments as to the Unseen are based upon supposed analogies with the seen. He says:—"clearly, the relation between the seen and the unseen universes cannot be the subject of any observation or experiment; since, by the definition of it, one term of the relation is absent." I do not know exactly what "mental peculiarity" Mr. Spencer exhibits in this statement. But it is a curious one. Am not I, the thinker, a part of the Unseen; no object of sense to myself or to others; and is not that term of relationship between the seen and the Unseen always present? But besides this, Mr. Spencer mistakes the object of the book in question. The theory there developed was not put forward as probable, its purpose was attained when it was shown to be conceivable and not inconsistent with any part of our present knowledge.

Mr. Spencer's second fault-finding is *appropos* of a Review of Thomson and Tait's *Nat. Phil.* (NATURE, July 3, 1879) by Clerk-Maxwell. Maxwell, knowing of course perfectly well that the authors were literally quoting Newton, and that they had expressly said so, jocularly remarked "Is it a fact that 'matter' has any power, either innate or acquired, of resisting external influences?" Mr. Spencer says:—"And to Prof. Clerk-Maxwell's question thus put, the answer of one not having a like mental peculiarity with Prof. Tait, must surely be—No." Mr. Spencer, not being aware that the passage is Newton's, and not recognising Maxwell's joke, thinks that Maxwell is at variance with the authors of the book!

Finally, Mr. Spencer attacks me for inconsistency &c. in my lecture on Force (NATURE, September 21, 1876). I do not know how often I may have to answer the perfectly groundless charge of having, in that Lecture, given two incompatible definitions of the same term. At any rate, as the subject is much more important than my estimates of Mr. Spencer's accuracy or than his estimates of my "mental peculiarities," I may try to give him clear ideas about it, and to show him that there is no inconsistency on the side of the mathematicians, however the idea of force may have been muddled by the metaphysicians. For that purpose I shall avoid all reference to "differentiations" and "integrations"; either as they



are known to the mathematicians, or as they occur in Mr. Spencer's "Formula." Of course a single line would suffice, if the differential calculus were employed.

Take the very simplest case, a stone of mass  $M$ , and weight  $W$ , let fall. After it has fallen through a height  $h$ , and has thus acquired a velocity  $v$ , the Conservation of Energy gives the relation

$$M\frac{v^2}{2} = Wh.$$

Here both sides express *real things*;  $M\frac{v^2}{2}$  is the kinetic energy acquired,  $Wh$  the work expended in producing it.

But if we choose to divide both sides of the equation by  $\frac{v}{2}$  (the average velocity during the fall) we have (by a perfectly legitimate operation)

$$Mv = Wt,$$

where  $t$  is the time of falling. This is read:—*the momentum acquired is the product of the force into the time during which it has acted.* Here, although the equation is strictly correct, it is an equation between purely artificial or non-physical quantities, each as unreal as is the product of a quart into an acre. It is often mathematically convenient, but that is all. The introduction of these artificial quantities is, at least largely, due to the strong (but wholly misleading) testimony of the "muscular" sense.

Each of these modes of expressing the same truth, of course gives its own mode of measuring (and therefore of defining) force.

The second form of the equation gives

$$W = \frac{Mv}{t}.$$

Here, therefore, force appears as the time-rate at which momentum changes; or, if we please, as the time-rate at which momentum is produced by the force. In using this latter phrase we adopt the convenient, and perfectly unmisleading, anthropomorphism of the mathematicians. This is the gist of a part of Newton's second Law.

The first form of the equation gives

$$W = \frac{M\frac{v^2}{2}}{h},$$

so that the same force now appears as the space-rate at which kinetic energy changes; or, if we please, as the space-rate at which energy is produced by the force.

Here are some of Mr. Spencer's comments:—"force is that which changes the state of a body; force is a rate, and a rate is a relation (as between time and distance, interest and capital); therefore a relation changes the state of a body."

The contradiction which Mr. Spencer detects here, and over which he waxes eloquent and defiant, exists in his own mind only. The anthropomorphism which has misled him is but a convenient and harmless relic of the old erroneous interpretations of the impressions of sense.

P. G. TAIT

#### COMET-FINDERS

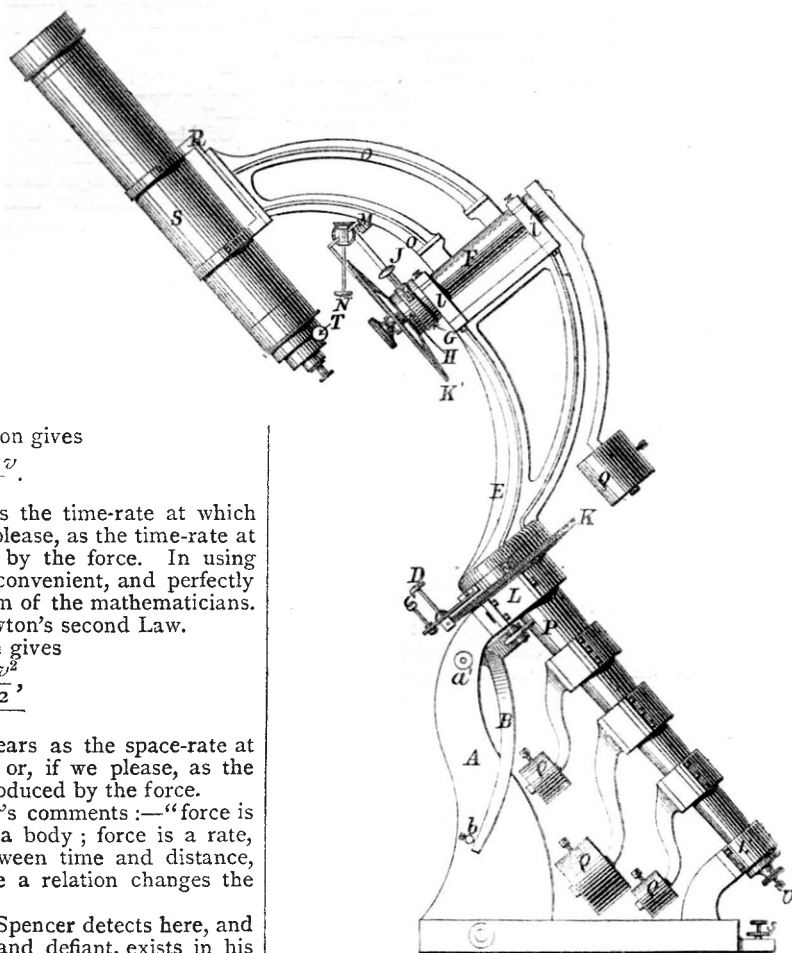
IT is only lately that the meteorites, or many of them which we see of a night making bright streaks in the heavens, have been shown to belong to definite streams

having definite orbits and periods, and with the increase of our knowledge of these orbits the number of comets identified as travelling in the same orbits as meteor-streams has likewise advanced.

Now that the intimate relation between comets and meteorites has been settled, greater interest attaches to the discovery of these casual visitors, many of which have passed in our neighbourhood unobserved. This is shown by the increased number of comets seen, now that it is part of the business of several observatories to keep up a systematic search.

To do this properly, a telescope of large field of view is required, and a constant sweeping of the heavens must be kept up, and to do this with an ordinary equatorial is extremely tedious, owing to the continual change of the position of the body required.

To go back to early days of comet-finding, we call to mind the first instrument specially constructed for the purpose, so far as we are aware. It is a telescope of Galilean construction, with an object-glass of  $2\frac{1}{2}$  inches



diameter, and having a total length of 5 inches. This was made by Dollond during the first few years of this century for Dr. Kitchener. Since that time astronomical instruments have grown apace, and we have now before us Dr. Carl's "Repertorium für Experimental-Physik" containing a description of the new comet-finder constructed by Herr Schneider for the Observatory at Vienna.

The telescope of this instrument has an object-glass of